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Assuming for the semi-diameter of the Sun the Greenwich value at the Earth's mean distance, the apparent semi-diameter during the comet's transit would be 15′ 46″·0, hence at the time of Stark's observation the comet's calculated position is 13′ 18″ from the Sun's eastern limb, and 14′ 17″ from his southern limb; which position, compared with the observation, shows a difference of 2′ 49″ in R.A., and 2′ 45″ in Decl. For the time of Pastorff's observation, corresponding to 19^h 26^m 54^s Greenwich mean time, we have from the above numbers—

	′	″
Comet East of Sun's Centre	2	32
Comet North „ „	6	31

which differs widely from the position assigned by Pastorff—6′ 10″ from the Sun's south-eastern limb.

The small effect of parallax is neglected in both cases.

I have been favoured by Mr. Ranyard with a copy of Pastorff's drawing, the original of which, as already stated, is in the Society's archives. The object represented as having been observed upon the Sun's disk is certainly a very remarkable one, bearing every resemblance to the ordinary figure of a telescopic comet, and quite different from the forms of sun-spots. If, however, Pastorff really saw the comet upon the solar disk, his account of its position must be very erroneous.

Stark's position also is open to some doubt, though much nearer to that which the comet must have occupied at the time of his observation; it may perhaps be granted that he possibly saw the comet upon the Sun. Nevertheless, it is impossible not to remark the great difference between his description of its appearance and that given by Pastorff. It appears very improbable that a comet seen in projection upon the Sun's disk would present the cometary aspect which Pastorff describes; it is far more likely that the nucleus only would be discerned. If Pastorff's observation is a *bonâ-fide* one, what he really did see must remain an enigma. I will merely add in conclusion, that there are other observations of his which are exceedingly difficult of comprehension.

Note on the Displacement of Lines in the Spectra of Stars.

By W. H. M. Christie, Esq.

In recent Numbers of the *Comptes Rendus*, P. Secchi has called attention to some apparent discordances between the Greenwich results for the motions of stars in the line of sight, as given in the *Monthly Notices* for November 1875, and those obtained by Dr. Huggins, and has with justice pointed out that there are many difficulties attending all such observations. On these

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points I wish to offer a few remarks, as the difficulties, to which P. Secchi refers, engaged my attention from the first: they were, in fact, pointed out to me by Dr. Huggins before spectroscopic observations were undertaken at Greenwich; and since that time much pains have been taken to perfect the adjustments of the spectroscopes, and to submit the pencil of rays from the comparison-light to identically the same conditions as those from the star.

In such a complicated instrument as the spectroscope, the errors of the many surfaces concerned are so combined with errors of adjustment of prisms, collimator, and telescope, that it has been a work of much labour to remove them one by one, though the peculiar form of the Greenwich Spectroscope has much facilitated the task. One important error in its bearing on this question appeared to be the astigmatism of the pencil after passing through the prisms (a defect which I have remarked in all spectroscopes that I have examined). This, after many attempts to improve the surfaces of the prisms by reworking, was corrected by means of a cylindrical lens within the collimator, and near the slit. I can hardly think that this error is caused by the surfaces of the prisms; but it may possibly have its origin in a variation of density in the glass, which is usually moulded. Another defect which gave much trouble was a slight wing to the lines (especially with a wide slit), which looked very like the error caused by tilt of a lens, but was ultimately traced to false light reflected from the base of the prisms. For this reason it is very important that the train be adjusted to minimum deviation, or rather to such a position that the pencil shall pass through the prism in a direction parallel to the base, and to the top and bottom. It is of no importance that the base should make equal angles with the two faces (though this is convenient for purposes of adjustment), for, with a pencil of parallel rays no astigmatism will be produced when the condition of minimum deviation is not satisfied. This condition, however, may serve as a convenient test of the parallelism of the pencil to the base of the prism; and, in fact, it was the want of such a test which made it difficult to adjust the "half-prism" with which the train begins. It was ultimately found, however, that, owing to greater density in the glass, the deviation through this "half-prism" was 5° greater than it should have been, and that, consequently, the pencil did not fall fairly on the successive prisms of the train, thus giving rise to false light on one side of the bright line viewed. This defect was corrected, and since last February the spectroscope appears to have been in perfect adjustment. But it appears to be of even more importance that the light which illumines the slit should form a pencil which just fills the whole of the object-glass of the collimator; for if the light fall obliquely on the slit, a false image may be formed by reflection from one of the jaws, or, perhaps in some cases, from within the collimator. I have seen a second or spurious pair of sodium lines formed in

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this manner by the side of the regular image; and Mr. Hilger informs me that he has frequently obtained a perfect image of the sodium lines from one jaw of a slit, when the other has been removed. It is difficult to trace the origin of such false light; but I have found that it disappears when the incident-pencil is properly adjusted, and can therefore only suggest some such cause as I have indicated above. It was for this reason that the method of comparison by means of a vacuum tube placed within the great telescope in the course of the rays from the star was given up, although, in Dr. Huggins's hands, it certainly worked well. It would seem probable that the systematic error remarked in the measures between August 1874 and March 1875, was caused by reflection of some of the light from the sides of a small rectangular tube placed in the axis of the collimator close to the slit (to cut off false light), and that thus a false image was formed; in any case, the two pencils from the star and comparison-light, passing through different portions of the collimator-object-glass and of the prisms, might be affected differently by optical defects. The method now adopted of throwing an *image* of the comparison-light on the slit by means of a collimating lens and small reflecting prism, appears to be free from the errors to which the use of a reflecting prism alone is liable; certainly no erroneous shift of the comparison-line has been detected since it was brought into use. The adjustment of the slit to the principal focus of the telescope is another important condition, which has been specially provided for in the Greenwich Spectroscope by means of a diagonal prism, which can be pushed into the course of the rays close behind the slit, allowing the latter to be viewed at the same time as the star by a suitable eyepiece. This arrangement serves as a finder, and also as a means to verify the position of the comparison-light and star on the slit, which can thus be reduced in width until it limits the star's image on both sides; this will be seen to be a necessary condition for accurate comparison, when it is considered that one-thousandth of an inch at the slit corresponds, with the Greenwich Spectroscope (as ordinarily used for star motions), to about 15 miles a second in the motion of a star, whilst with such an instrument as P. Secchi's it would represent a much larger quantity. If P. Secchi had not used a sufficiently narrow slit, the appearances he describes of the star-line shifting its position when the driving clock of the equatoreal was stopped (allowing the star to travel across the slit) would be exactly accounted for.

I have entered into some detail on these matters, as the brief statement given in the *Monthly Notices* for last November seems to have been imperfectly understood, whilst P. Secchi apparently does not admit the propriety of rejecting the measures prior to 1875, May 31. These observations were given in order to show fairly the errors to which the method is liable, and the results could in any case only be considered as preliminary. In such an inquiry much experience is required, and large errors are there-

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fore to be expected in the earlier measures. The observations, however, since the new method of comparison was adopted show few discordances of *sign*, and the measures of the *magnitude* of the displacement agree as well as can be expected, considering how small the quantity to be measured really is, and that before the bright line micrometer was introduced (1875, November 29) it was very difficult to see the pointer and star line at the same time.

Notwithstanding these difficulties, it is gratifying to find that, out of the list of 21 stars which have been observed, both by Dr. Huggins and Mr. Maunder, there are only two cases of discordance, as will be seen from the following table; and for both of these stars Dr. Huggins has expressed himself as dissatisfied with his observations: whilst the Greenwich results for these stars rest on too few observations at present.

Motions in the Line of Sight.

+ denotes Recession; − Approach.

Star.	Huggins.	Greenwich.
α Andromedæ	−	− 35
Aldebaran	+ ?	+
Capella	+	+ 20
Rigel	+	+
Betelgeuze	+ 22	+ 75
Sirius	+ 18 to 22	+ 25
Castor	+ 23 to 28	+ 25
Procyon	+	+ 40
Pollux	− 49	−
Regulus	+ 12 to 17	+ 30
β Ursæ Majoris	+ 17 to 21	+
α Ursæ Majoris	− 46 to 60	−
β Leonis	+ ?	−
Spica	+	+
η Ursæ Majoris	+ ?	−
Arcturus	− 55	− 35
ε Boötis	−	−
α Coronæ	+	+
Vega	− 44 to 54	− 37
α Cygni	− 39	− 50
α Pegasi	−	− 27

In the above Table, in cases where the sign only is given, the observations are at present insufficient to determine the amount of the motion with any degree of certainty.

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With regard to the amounts of the displacement, P. Secchi appears to be under a misapprehension, as it is in terms of the width of the narrow bright line of the vacuum tube, not of the broad line in the star, that the displacement is estimated. The great breadth of the latter in some stars (especially *Altair* and *Sirius*) constitutes one great difficulty of the observation, and the discordances in the case of *Altair* are, doubtless to be attributed to this cause, since a small error in the estimation of the centre of a broad band may easily be committed. Such a source of error will be sufficiently indicated by the discordances of the individual observations, independently of the notes of the observer; but there remains the question of the fixity of the centre of the F line as the pressure of the gas is increased. Dr. Huggins has fully discussed this point; and I need only refer to it to add that, by his kindness, I have had the opportunity of seeing his experimental verification of this fact. Independently of this, the fact remains that there is a relative displacement of the F line in the case of stars for which the breadth of the line is sensibly the same; besides which similar displacements are shown by the *b* lines (which are comparatively narrow), and accordant results have been obtained for the motion of *Arcturus* from the D, *b*, and F lines. An examination of the results for different stars on the separate nights of observation will show that the motions of approach and recession are fairly distributed; whilst the comparisons of the F line in the spectrum of the Moon (for which the motion is insensible) show conclusively that there is no systematic error arising from maladjustment of the spectroscope. In all the measures the greatest care has been taken to avoid bias, the observer being careful to make the bisection of the line in the spectrum of the star or Moon before that of the comparison spectrum was seen, and having generally no previous knowledge of the direction in which a displacement was to be expected. No observation has, under any circumstances, been suppressed, and consequently some of the measures might fairly be rejected on the ground of erroneous bisection.

Royal Observatory, Greenwich,
1876, *May* 12.